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A RAND NOTE

RAND Workshop on Antiproton Science and Technology, October 6–9, 1987: Annotated Executive Summary

Bruno W. Augenstein

October 1988

40 Years 1948-1988

RAND

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This Note describes a conference held in October 1987 to review the critical issues surrounding the establishment of a comprehensive U.S. antiproton research program and to help formulate its research goals. The conference was organized around three major themes: (1) basic machine, facility, and scale-up review--antiproton production and collection; (2) a basic physics program for a low-energy antiproton source in North America; and (3) near-term and precursor applications using an initial low-energy antiproton source. Among the major conclusions were the following: The United States can construct an intense source of low-energy antiprotons in three to four years, and develop portable antiproton storage devices (rings and ion traps). A dozen classes of key low-energy antiproton experiments can be conducted on questions ranging from charge parity violation to condensed matter. A number of near-term important applications are possible using the source and portable storage devices.

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PREFACE

The United States Air Force Project Forecast II identified use of antiprotons as one of a number of highly promising technologies. This finding prompted close examination of antiproton science and technology. The RAND Corporation, through its Technology Applications Program within Project AIR FORCE, in association with AFSC and the USAF Astronautics Laboratory, performed technical evaluations of antiproton science and technology.

It quickly became evident that a necessary initial task was to gather together a comprehensive picture of the required first steps in the research paths leading to broader understanding of the "state-of-the-art" in this field. A major issue is how to bolster the fundamental scientific base to rapidly increase the ability to access the near term and longer term promise of antiproton research.

As part of this effort, RAND organized two conferences bringing together leading scientists and technologists, to review what is known, and what is needed to find out, about antiproton science and technology. The first conference, in April 1987, was held to identify critical issues. The second, larger conference, in October 1987, was intended to review the critical issues at a depth adequate to help formulate goals and research objectives for a sound, comprehensive U. S. antiproton research program within the next decade. A detailed *Proceedings* was prepared from this conference.

This Note summarizes the background for, and accomplishments of, the second conference, running from October 6 to 9, 1987. The second conference was organized as a Workshop in which three major themes were addressed. The findings of the Workshop, summarized in this Note, provide an initial basis for a comprehensive near-term program of antiproton research in the United States. Participants believe the research promises to result in both compelling basic physics rewards and critical insights into a number of technology applications possibilities, at a pace whose early accomplishments may surprise many.

The complete Workshop *Proceedings* in book form was reviewed as unclassified and cleared for open publication by OASD-PA, Department of Defense; this Executive Summary of the Workshop is accordingly treated in the same way.

SUMMARY

The October 6-9, 1987 RAND Workshop on Antiproton Science and Technology was organized around three major themes, each addressed by a group of Workshop participants:

Group I - Basic machine, facility, and scaleup review: antiproton production and collection (RAND rapporteur, E. Harris)

Group II - Basic physics program for a low-energy antiproton source in North America (RAND rapporteur, P. Rehmus)

Group III - Near-term and precursor applications using an initial low-energy antiproton source (RAND rapporteur, J. Dewar)

The background of the Workshop, brief annotated summaries of the presentations, and significant findings by each group are the central focus of this Note.

Some major observations of the Workshop include:

- The United States can construct an intense source of low-energy antiprotons in three to four years, delivering approximately 10^{14} low-energy antiprotons/year. A next level of antiproton production and collection scaleup could be provided via existing proposals for advanced hadron/kaon facilities.
- An R&D program can be formulated to investigate several options for achieving further scaleups to milligrams/year of low-energy antiprotons.
- The technology exists to develop portable antiproton storage devices (rings and ion traps), allowing antiproton transport to, and use at, any suitable laboratory.
- The physics case for a low-energy antiproton source in North America is most alluring, with great potential for new and unexpected discoveries.
- About a dozen classes of key low-energy antiproton experiments were identified, ranging over a great variety of questions, from charge parity violation studies to condensed matter studies.

- The CERN/LEAR facility will continue to only scratch the surface of important low-energy antiproton research, emphasizing strong motivations for a North American antiproton source in addition to LEAR.
- The proposed low-energy antiproton source in North America that supports the basic physics programs will concurrently support a number of antiproton applications-related technology programs.
- These technology programs include possible small tools to study extreme states of matter; a propulsion test facility for investigating antiproton engine concepts; new, improved techniques for biomedical imaging, therapy, and tissue analysis; and other diagnostic and instrumentation analysis tools.
- Even prior to the availability of a North American low-energy source, a number of simulations and calibrations relevant to antiproton science and technology can be made using normal matter.
- Availability of a North American low-energy antiproton source opens the possibility for rapid progress in realistically assessing the basic feasibility/utility of many proposed antiproton applications.

We have published, in full, the papers presented at the Workshop: *Proceedings of the RAND Workshop on Antiproton Science and Technology*, World Scientific, Singapore, New Jersey and Hong Kong, June 1988, 759 pages. The papers reflect final and up-dated versions of papers presented at the Workshop. Interested readers are urged to review the individual technical papers in the *Proceedings*; the papers span a broad range of scientific and technological fields. In a few cases, the published papers cover verbal presentations given at the Workshop; in other cases verbal presentations did not result in finished papers. We anticipate that the Workshop materials may be used as a basis for a number of individual proposals to funding agencies in the near future, for support of the basic North American low-energy antiproton source, enabling tools, and experimental programs.

The participants in this Workshop (and in the April conference) were able to convey the excitement and promise of near-term programs using low-energy antiprotons. By near-term we mean a five-to-seven-year period following availability of a North American low-energy antiproton source, as prescribed by the Workshop. The extensive representation of diverse groups — from universities, national laboratories, governmental organizations, major hospitals, U.S. industry, and scientific staff collaborating in international physics programs — is evidence of the rapidly growing interest in low-energy antiproton research. This interest suggests that, given appropriate support, significant near-term results are within our grasp.

As one output of the Workshop, a representative basic antimatter RDT&E program was constructed. This suggested program, to be pursued by a consortium of partners, is briefly summarized in the latter part of the section on Workshop Background and Retrospective.

ACKNOWLEDGMENTS

The RAND Corporation wishes to thank the co-editors of the Workshop *Proceedings* — B. Bonner, Rice University; F. Mills, FNAL; and M. Nieto, LANL — for their invaluable help in structuring the meeting, presenting review papers, and obtaining the best talks and papers from the participants. Additional thanks for their special and essential contributions to the Workshop go to Captain W. Sowell, AFAL, and to Drs. H. Mayer, J. Dewar, E. Harris, P. Rehmus, and Ms. O. Stauber of RAND.

CONTENTS

TABLES

GLOSSARY

AFAL	Air Force Astronautics Laboratory
AFSC	Air Force Systems Command
AGS	Alternating Gradient Synchrotron
BNL	Brookhaven National Laboratory
CERN	European Organization for Nuclear Research
CERN (ACOL)	Centre Europeenne pour la Recherche Nucleaire (Antiproton Collector)
CP	Charge parity
CPT	Charge parity time
CT	Computer tomography
DT	Deuterium tritium
FNAL	Fermi National Accelerator Laboratory
HF	Hadron Facility
LAMPF AHF	Los Alamos Meson Physics Facility, Advanced Hadron Facility
LANL	Los Alamos National Laboratory
LEAR	Low Energy Antiproton Ring
NBS	National Bureau of Standards
OASD-PA	Office of the Assistant Secretary of Defense-Public Affairs
QCD	Quantum chromodynamics
T	Tritium

I. WORKSHOP BACKGROUND AND RETROSPECTIVE

A central focus of the Workshop on Antiproton Science and Technology, October 6-9, 1987, at The RAND Corporation, was development of the case for a multi-user low-energy antiproton source in North America. The case is premised on the complementary possibilities for major basic science of low-energy antiprotons from such a source, and on concurrent applications research such a source can support. Some possibilities emerged as significant research paths from the April Antiproton Conference. These two major kinds of uses will employ similar tools and techniques, and certain facilitating technologies (in particular, portable antiproton storage devices) will play a prominent role.

A low-energy antiproton source, other enabling tools, and the several important sciences and RDT&E programs such a source can support appear necessary from several points of view. Without such tools, many uses of antiprotons, and in particular macroapplications possibilities, will remain idle speculations. Hands-on experience with low-energy antiprotons will be necessary to develop the research infrastructure to support any adequately broadened U.S. science and applications efforts. Such research appears critical to goals of establishing with confidence which macroapplications are possible and of long-term importance. Portable antiproton storage devices will allow hands-on experience with antiprotons virtually anywhere in the United States, e.g., in university laboratories.

There is a reasonable expectation that in the long term the high specific energy storage per unit mass ($2C^2$) of antiparticles will be uniquely utilizable in macroapplications such as mass propulsion or compact meterable energy release. Specific ideas have been proposed.

Thus, we understand that if we had one gram of antimatter available we might achieve — using a variety of conceptual engine types — a range of rocket missions roughly encompassed by the simple relation $M(\Delta V)^2 \sim 10^5$, where M is the payload mass in metric tons and ΔV is the mission velocity increment desired, in km/sec. One gram of antimatter could put a 1000 ton payload into a Mars mission, or allow a 100 kg payload to achieve 1000 km/sec velocities. Analogous results are obtained for air-breathing engines; i.e., one might achieve increases of factors of 3 to 5 or more in payload/gross weight ratios by clever use of antimatter to amplify the specific impulse of standard engine cycles. Of course, advanced propulsion concepts other than those employing antimatter have been discussed, such as the exploitation of a number of fusion possibilities. But among the attractions of antimatter appear to be the energy release available, the ability to do very near term definitive experimentation, and the promise of a far wider class of relatively lightweight engine concepts. Thus, while advanced propulsion concepts in general have many uncertainties, those concepts using antimatter can be experimentally addressed using a facility such as is described in two papers summarized subsequently (papers by Morgan and Callas).

Although continued search for new concepts and further exploratory research is certainly necessary in the macroapplications area, it is too early to believe that important defensible conclusions as to technical and economic viability could be made with our present knowledge base. The attitude of the Workshop therefore was that one keeps an eye on future possibilities, but concentrates on those early achievable steps in antiproton sources and technology for handling antimatter which can give experience needed for promulgation of a sensible RDT&E program. Meanwhile, those steps which exploit the quality and composition of the energy released by antiparticle annihilation, rather than simply the gross quantity of energy available per unit mass, already appear to have enough potential utility in basic science, and in technology, materials research, and medical research, to justify the investments of money and intellectual effort that they entail.

In effect, the Workshop took the position that instead of now stressing the possible ultimate macroapplications (whose discussion is currently almost entirely without a sound and comprehensive experimental basis), we must initially stress the importance and productivity of the first phase (about 10 years duration) of an antiproton RDT&E program. That phase provides core knowledge on which to base the theories and experiments which will make more precise what basic steps are feasible, and whether macroapplications are feasible; and, if so, sensible paths to achieve these feasible steps. We continue, accordingly, to explore antiproton production scaleup issues as a decisive factor of macroapplication feasibility. This constituted an additional focus for the Workshop.

It appears that at antiproton levels which could be made available by an initial U.S. low-energy source, the interests of science users and the first step interests of applications users could both be satisfied. In addition to developing the motivations for a U.S. low-energy antiproton source, the Workshop discussions were alert to the possibilities for a *consortium of users* to support the construction of a U.S. low-energy antiproton source, and to support the RDT&E programs which can be based on the availability of such a source. The broad nature of the programs to be undertaken using the low-energy antiproton source suggests that *foreign members* of the consortium could well be involved.

To satisfy these Workshop Aims, concurrent discussions were organized into three basic groups of topics, identified by Roman numerals I, II, and III.

The following outline of the discussions is amplified in greater detail in following sections of this Executive Summary of the Workshop. Individual papers are generally referenced by citing the paper number in each group — e.g., the paper by D. Peaslee is paper I1.

Group I was to consider fundamental issues of production and collection of antiprotons. Consideration was first to be given to options, characteristics, and schedules for a near-term North American low-energy antiproton source, to be based in U.S. sites at Brookhaven National Laboratory or Fermi National Accelerator Laboratory, and capable of delivering of the order of 10^{14} low-energy antiprotons per year, or more, at energies suitable for both fundamental physics and applications experiments. It was recognized that special earlier capabilities might be considered, giving us fewer low-energy antiprotons, but the level noted remains as the important goal. Next, consideration was to be given to the feasibility of small transportable antiproton storage rings, storing antiprotons at typical energies of tens of MeV, for antiproton delivery at any suitable laboratory site. Such rings were to be filled with antiprotons at the low-energy antiproton source.

Finally, issues of scaleup were to be addressed, in two stages: first, the level of scaleup potentially available if one utilizes the advanced hadron/kaon facilities now being proposed, and second, the additional scaleup potentially available by fundamental machine considerations of production, collection, and cooling (in effectively real time). We know that considerable RDT&E is vital to achieve the latter level of scaleup (with which we would achieve the milligrams per year level). Speculations on means to achieve ~gram/yr production were also voiced. A first cut at these RDT&E issues was a goal for Group I activities.

Group II was to consider the basic physics programs accessible with the delivery potential of a near-term North American low-energy antiproton source (i.e., of the order of 10^{14} antiprotons per year).

The science case for a U.S. (North American) low-energy antiproton source is critical for adequate development. We believe that a remarkably broad science program was discussed at the Workshop. Antiproton science and the science experiments will provide major incentives for a U.S. antiproton source, as well as an essential technical infrastructure for rapid closing of information gaps now inhibiting confident assessment of the possibilities and merits of many applications. The science base should be a strong attractor for interesting the scientific community in antiproton research.

The array of experiments feasible with the low-energy antiproton source is impressively large. Group II was accordingly to consider a diverse and multi-disciplinary set of programs, including classes of experiments relevant to:

- Tests of invariance principles
- Antiproton annihilation in nuclei
- Gravity and antiprotons
- Antihydrogen and basic physics tests
- Antimatter cluster ions, and other atomic/molecular issues
- Meson spectroscopy
- Antiprotons and condensed matter (storage in normal matter, etc.)
- Antiproton studies at momenta up to several GeV/c

Group III was to consider a range of applications-related issues for which experiments could be carried out using the number of antiprotons deliverable from an initial North America antiproton source (i.e., again of the order of 10^{14} antiprotons/year).

Accordingly, the topics to be addressed included:

- Design of portable ion traps capable of accepting antiprotons at about 50 KeV. The quantity of antiprotons storable would be scalable to about 10^{13}, commensurate with intended experiment/applications purposes.
- A "table-top" high pressure/high temperature/high particle flux testing tool, using as a source antiprotons stored in small rings or traps.
- A prototype tool for exploration, testing, and development of a new and revolutionary class of medical imaging and therapy procedures.
- A facility for initial testing and screening of a range of interesting design concepts for antiproton propulsion and energy storage, providing for "hands-on" testing of ideas in this area.
- Exploration and development prospects, where useful, of classes of scientific and commercial diagnostic probes, tools, and special techniques.

These applications paths can make a reasonable complementary case, along with the basic science programs of Group II, for a low-energy antiproton source. Many tools common to those for science programs will be applicable. A goal is to make evident support for a North American low-energy antiproton source by the strong *dual motivation* of the science programs outlined by Group II and the initial applications explorations discussed in Group III activities.

In summary, there were several themes for this Workshop.

a. First, we want to make as forceful a case as we can for motivating an initial U.S. (North American) low-energy antiproton source, by emphasizing the depth of the science base accessible (Group II).
b. Second, we want to emphasize that an initial U.S. antiproton source of the scale we contemplate (10^{14} antiprotons per year) can also, in a complementary way, develop useful applications paths and give us insights for evaluating future antiproton uses (Group III).
c. Third, we believe that solution of applications-related problems at an experiment base compatible with 10^{14} low-energy antiprotons per year is a necessary condition to help establish feasibility of other applications.
d. Fourth, should no fundamental principles against using antimatter on a much larger scale emerge, the scaleup activities of Group I are intended to give us insights on how antimatter could be made available in larger amounts.

We believe several key findings emerged from the Workshop:

- The United States can *speedily* construct an intense low-energy antiproton source, delivering approximately 10^{14} antiprotons per year, and *concurrently* engage in fundamental investigations of the *scaleup possibilities* to deliver much larger amounts of antiprotons.
- The *science* uses for a North American intense low-energy antiproton source are *very broad*, and its physics exploration potential *most compelling*; the CERN/LEAR capability will touch on only a *small part* of the diverse experiments accessible. Portability of antiprotons is also important.

  The comments on CERN/LEAR are reinforced by comparing the antiproton amounts desired by certain experiment classes (see Table 2 in Group II Activities Summary) with the CERN/LEAR antiproton allocations cited in the paper by Peaslee (paper 11). Additionally, there is a need for applied science experiments which could have considerable difficulty of justification in facilities whose exclusive mission is basic physics.

- The *same* low-energy antiproton source vital for North American basic physics with antiprotons can be used for exploration and development of initial *applications technology* and give us realistic *near-term* assessments of future applications potentials for antiprotons.

If such findings are followed up, there is every reason to believe that remarkably fast progress is possible for antimatter research, and that we will be in an excellent position to evaluate critically the long-term possibilities in a much shorter time than is often assumed.

To pursue this general conclusion, the Workshop results and discussions were used to construct a representative national antimatter RDT&E program. An initial interlinked 10 year program was considered whose primary goals were to conduct the large array of experiments possible which could realize the enormous promise of antimatter science research, on one hand, and to develop concurrently the technology base to assess the reality and promise of many macroapplications, on the other hand. The RDT&E program was comprised of five essential elements:

- Construct a North American intense low-energy antiproton source
- Develop classes of portable antiproton storage devices
- Provide several specialized applications laboratory capabilities
- Support broad science, applied science, and technology development experiments
- Tackle antiproton production/collection scaleup issues seriously

A suitable five-element program of this sort was estimated to cost a total of about $400 million over a 10 year period. The first five years of the 10 year program would cost about $125 million. These costs include funding levels of about $4 million, $11 million, and $15 million in years 1, 2, and 3, respectively, to invest in the critical tools of the intense source and portable storage. The broad experiment program was allocated nearly one half of the total 10 year funding.

The overall allure and promise of the proposed antimatter RDT&E program appear to support the notion of a consortium of interested parties to invest in and use the results of the program, and to share the burdens appropriately. The suggested funding level appears to be a reasonable middle ground. Significantly lower levels begin to run into problems of unduly stretching out assessments of applications possibilities and increased reliance on the uncertain role of overseas research centers; significantly higher funding levels could run into inefficiencies because of the demands to increase the community of researchers involved excessively rapidly.

Finally, there are a number of consortium considerations to support such an antimatter RDT&E program. One possible representative sharing arrangement, emphasizing a few primary interests, might be:

DoD/USAF - Propulsion; meterable power; specialized applications

DOE - Applied materials science; equation of state, opacity measurements, etc; specialized applications

NIH - Medical research and applications

NSF - Support of basic science using low-energy antiprotons

NASA - Propulsion; meterable power

Industry - Applications support (e.g., support of specialized laboratories)

A great deal of this work can be carried out in academic laboratory settings, and at other sites remote from the source, exploiting portable antiproton storage devices. The provision of the enabling tools would likely be a governmental responsibility. Once such tools are in place, industry participation in a number of basic and applied fields is a very real possibility. Indeed, there appear to be near term uses of antiprotons offering prospective commercial attractions: see for example topics discussed in papers III10, III14, and other applications noted *in Group III discussions*.

Underlying a program to find applications in these fields and to realize the primary interests of partners such as the above is an extensive effort in basic science, such as is reflected in much of the discussion of the October 1987 Workshop. Without that effort, the information base to evaluate applications promise and utility will be generally absent. With that effort, an enormous array of important science, new discoveries, and exciting physics results await — and early applications testing is expected to find broadly useful and immediately practical tools.

II. RAND WORKSHOP ON ANTIPROTON SCIENCE AND TECHNOLOGY OCTOBER 6-9, 1987

A. Workshop divided into three major groups:

Group I: Machine issues — Production, Collection, Scaleup

Group II: Basic science, using an initial Low-Energy Facility (LEF)

Group III: Near-term and precursor applications, using an initial LEF

B. Formal Talks/Presentations/Formal Papers, by group and by program order:

Group I:

1. Potential Low-Energy Antiproton Sources in the United States
   D.C. Peaslee (University of Maryland)
2. Low-Energy Antiproton Possibilities at Brookhaven National Laboratory (BNL)
   Y.Y. Lee, D.I. Lowenstein (BNL)
3. The AGS Complex as an Antiproton Filling Station
   Y.Y. Lee, D.I. Lowenstein (BNL)
4. Scaleup of Antiproton Production and Collection
   D.J. Larson (UCLA)
5. Multiple Collision Effects on Antiproton Production by High-Energy Protons (100 GeV-1000 GeV)
   H. Takahashi, J. Powell (BNL)
6. BNL - Fermi Laboratory (FNAL) Antiproton Source Comparison
   F.E. Mills (FNAL), Y.Y. Lee (BNL)
7. Scaleup of Antiproton Production Facilities to 1 mg/year
   F.E. Mills (FNAL)
8. Transportable Storage Ring for Antimatter Transport, and Study of Antimatter Interactions
   D. Cline (UCLA)
9. An Advanced Hadron Facility: Prospects and Applicability to Antiproton Production
   T. Goldman (Los Alamos National Laboratory)
10. An Advanced Kaon Facility - A Next Step to Antiproton Production
    E. Blackmore (TRIUMF - Canada)

11. Discussion - Potential Research and Development Areas for Large Scaleup to Antiproton Production Rates of ≥ 1 mg/year
    F.E. Mills (FNAL)

Group II:

1. Basic Physics Program for a Low-Energy Antiproton Facility
    B.E. Bonner (Rice University), M.M. Nieto (Los Alamos)
2. Antiproton Annihilation in Nuclei
    G.A. Smith (Penn State University)
3. Particle Emission from Antiproton Annihilation at Rest in Uranium
    G.A. Smith (Penn State University)
4. Meson Spectroscopy - Annihilations into Exotica
    S. Sharpe (Stanford Linear Accelerator Center, SLAC)
5. Invariance Principles - Antiproton tests of CP, CPT and T
    J. Miller (Boston University)
6. Gravity and Antiprotons: $g(\overline{P})/g(H^-)$
    M.M. Nieto (Los Alamos)
7. Normal Matter Storage of Antiprotons
    L.J. Campbell (Los Alamos)
8. Antihydrogen Production Schemes
    J.B.A. Mitchell (University of Western Ontario)
9. Synthesis of Large Cluster Ions from Elementary Constituents - Possible Route to Bulk Antimatter
    W.C. Stwalley (University of Iowa)
10. Bibliography of Hydrogen Cluster Ions
    W.C. Stwalley (University of Iowa)
11. Production of Heavy Antinuclei: Review of Experimental Results
    R.L. Forward (Hughes Research Laboratories)
12. The Standard Model and its Problems: The Physics Background for an Advanced Hadron Facility
    T. Goldman (Los Alamos)
13. Antimatter - History and Properties
    M. Nieto, R. Hughes (Los Alamos)

Group III.

1. Portable Antiprotons - Traps that Travel
   M. Hynes, S. Howe (Los Alamos)
2. Extreme States of Matter: Could Antiprotons Be Used To Power Table-top Equation of State or Opacity Experiments?
   J.C. Solem (University of Illinois at Chicago)
3. (Addendum to 2.) Table-top Ceneration of External Particle Fluxes
   H. Mayer (RAND Corporation)
4. Propulsion Test Facility - Antiproton Stopping and Annihilation in various Antimatter Engine Types - Needed Experimental Information
   D. Morgan (Livermore Laboratory) et al. (contributions)
5. Antimatter Spacecraft Propulsion Experiments on Energy Deposition with Current Antiproton Production Rates
   J.L. Callas (Jet Propulsion Laboratory)
6. Available Annihilation Energy in Gas-Core Engines
   B.N. Cassenti (United Technologies Research Center)
7. Boosting Annihilation Energy with Muon - Catalyzed Fusion
   J. Rafelski (University of Arizona)
8. Boosting Annihilation Energy with Muon - Catalyzed Fusion
   H. Takahashi (BNL)
9. Experiments in Hydrogen Ion Facility - Neutral Particle Beam Propulsion Experiments
   V.E. Haloulakos (McDonnell Douglas Astronautics Company)
10. Biomedical Potential of Antiprotons
    T. Kalogeropoulos (Syracuse University)
    L. Gray (Syracuse University)
    R. Muratore (Syracuse University)
    G. Bennett (BNL)
    D. Bassano (Department of Radiology, SUNY Health Science Center)
11. Stopping Power - Compounds, Tissues
    A.M. Koehler (Harvard Cyclotron Laboratories)
12. Categories of Clinical Applications To Be Considered
    J. Archambeau (Loma Linda University Medical Center)
13. Antiprotons for Probes, Tools, Instrumentation, and Special Techniques
    E. Ottewitte (Idaho National Engineering Laboratory)

14. Potential Applications of Antiprotons for Inspection and Processing of Materials
    L.B. Greszczuk (McDonnell Douglas Astronautics Company)
15. Antimatter Science and Technology Bibliography (August 1987)
    R.L. Forward (Hughes Research Laboratories)
    - Production and Collection of Antiprotons
    - Production of Heavy Antinuclei
    - Production of Low-Energy Antiprotons
    - Production of Antihydrogen Atoms, Molecules, and Clusters
    - Slowing, Cooling, Trapping of Atoms, Ions, and Molecules
    - Low Energy Antiproton Annihilation Processes
    - Non-Propulsion Applications of Antimatter
    - Antimatter Propulsion
    - Conference Proceedings
    - Antimatter News and Popular Articles

III. SUMMARY OF GROUP I ACTIVITIES, REFERENCED TO NUMBERED PRESENTATIONS

Papers 1, 2, 3, and 6 were devoted to the provision, in the near term, of a low-energy antiproton source in the United States.

Paper 1, by Peaslee, reviewed how FNAL and BNL could serve as a source of low-energy antiprotons, and compared these sources with the European CERN facility (LEAR) as a model. Both antiproton production and delivery of the antiprotons at low energy are treated. The improved antiproton source at CERN (ACOL) may produce up to 10^{12} antiprotons per day, but the LEAR duty cycle is such as to result in $\sim 10^{13}$ low-energy antiprotons per year. Because there are other users of LEAR, the paper estimates the very-low-energy ($\leq$ 50 KeV) antiprotons available from LEAR might be $\leq 10^{12}$ antiprotons per year. Several machine options are possible at FNAL, giving a range of $\sim 10^{13}$ to several times 10^{14} antiprotons per year available at 9 GeV/c and suitable for delivery to lower energies ($\leq$ 50 KeV). Delivery at $\leq$ 50 KeV would be possible via several schemes at no significant loss of antiprotons, so that $\sim 10^{13}$-10^{14} antiprotons per year might be delivered at $\leq$ 50 KeV. Costs for this goal, going the simplest low cost route, might be ~20-50 million dollars, over a 4-5 year period. BNL currently has no dedicated antiproton source, but one could evolve from the ongoing Booster project in three to four years. The BNL source possibilities, schedules, and costs are described in detail in paper 2, by Lowenstein and Lee, and in paper 3, by Lee and Lowenstein. Using realistic duty cycles, at a cost of about 9 million dollars, about 10^{14} antiprotons per year become available; however, at BNL one can also purchase additional accelerator time (for ~$100,000/week), and with dedicated time get up to $\sim 5 \times 10^{14}$ antiprotons per year at momenta of 4 GeV/c. A further ACOL-type enhancement at BNL might in the future raise production to ~several x 10^{16} antiprotons per year. For delivery at BNL, one could take a no-cooling approach, but accept a 10^4 factor loss in the beam, bringing the yield to perhaps 10^{11} - 5×10^{11} antiprotons per year at 10 KeV. Provision of substantial cooling would give a loss factor of $\leq$ 10 to 20 KeV, at an additional cost of 5-6 million dollars in three years (concurrent with the Booster construction). In this way, BNL might obtain $\geq 5 \times 10^{13}$ antiprotons per year at 20 KeV, in three to four years at a minimum estimated cost of perhaps 15 million dollars. For planning purposes, a prudent cost estimate, prior to a detailed proposal, might be 25 million dollars. Paper 6, by Mills and Lee, compared BNL and FNAL antiproton source characteristics in detail. Thus the United States has several routes to a near-term low-energy facility. Papers in Groups II and III suggested powerful motivations for aiming at the high end of the accessible low-energy antiproton delivery rates (i.e., $\sim 10^{14}$ rather than $\sim 10^{11}$ antiprotons per year, implying cooling at BNL, even though a U.S. capability for $\sim 10^{11}$ antiprotons per year at ≤ 50 KeV would permit a major step forward in antiproton science and technology RTD&E). Bonner and Nieto, in paper II1, Table II, summarized the number of antiprotons required for new/more precise experiments in 12 science areas. The numbers range from a few antiprotons to $> 10^{14}$ antiprotons, with many of the areas needing $\geq 10^{12}$. Note also that one approved CERN experiment (PS195, charge parity violation) is scheduled for a total of 10^{13} antiprotons.

Along with provision of a U.S. facility as an antiproton filling station, transportable antiproton storage devices are important, for they allow antiprotons to be delivered to laboratories for in situ use. One such transportable device was taken up in paper III1. Another implementation was considered by Cline in paper I8: a portable storage ring. The ring is used generally as an antiproton source, with no experiments normally carried out in it. At a weight of ≤ 10 tons, and with dimensions of $\sim 4.4 \times 2.4$ meters, a ring storing $\sim 10^{10}$ - 10^{11} (possibly to 10^{12}) antiprotons seems feasible, with particle lifetimes of ≥ 3500 hours with cooling (≥ 100 hours without cooling), and capable of a kinetic energy range of ~ 100 MeV - 200 KeV, using superconducting technology. Paper I11 raised the possibility of substantial scaleup of the number of stored antiprotons. Work is needed on the superconducting magnet. An emergency beam dump into the magnet as a safety measure looks feasible. The proposed design is based on a design base of a number of low-energy storage rings, particularly the LEAR-ELENA proposal, called SELENA (Superconducting ELENA). The experiments using such a ring are any requiring significant momenta. A partial list would include medical applications - paper III9; annihilation phenomenology - papers II2 and II3; nuclear physics tests - papers II4 and II5; table-top tools - paper III2; and a variety of applications falling into the categories of papers III12 and III13. A transportable storage ring is one of the key enabling tools permitting use of antiprotons in industry, university,[1] and national laboratories, remote from the basic antiproton source.

Papers 9, by Goldman, and 10, by Blackmore, discussed the potential scaleup for antiproton production and collection provided by an advanced hadron or kaon production facility. The physics case for such a facility is compelling, and is described here and in paper II13, by Goldman. The physics uses include hadron spectroscopy, kaon decays, hypernuclei, neutrino physics, special proton physics, and other physics of electroweak and

[1]Uses include filling *existing* rings, furthering widespread antiproton research.

strong interactions. Four proposals exist for such a facility: Canada (TRIUMF); United States (LAMPF AHF); European Hadron Facility; and Japanese Hadron Facility. These are machines in the 30-60 GeV energy, ~50-100 μA current range. It seems probable that at least one such machine may be built. Used as an antiproton source to reach higher production and collection rates, current technology would need extensions in target design, collection, debunching, and cooling. The papers suggested that one could produce, collect, and cool perhaps 10^9 - 10^{11} antiprotons per second, giving a factor of ~10^3 - 10^4 scaleup over the yields "immediately" available from FNAL or BNL. That is, the scaleup would take us from the nanogram level to the several micrograms per year level. The earliest such a machine might be available is in the mid 1990s. While antiproton production and collection are not a primary motivation for such an advanced hadron/kaon machine, the facility would allow significant increases in antiproton yield, and would serve as a partial test bed for still larger scaleups. One might very well think of adding a low-energy antiproton facility to such a machine complex, if an accessible one is built soon enough, as a possible alternative to going through an ACOL-like program at, say, BNL.

Papers 7, by Mills, and 5, by Takahashi and Powell, took up general issues of machine scaleups to produce and collect of the order of ~10^{14} antiprotons per second, giving annual yields in the few milligrams range. Paper 7 covered general topics of production issues, collector and accelerator types, candidate accelerators, antiproton cooling methods, and potential research and development areas (the latter topic was elaborated in presentation 11, by Mills). In each of the topics noted, a number of critical discrete issues were treated. For example, in the section on antiproton cooling methods, the discussion covered stochastic cooling, electron cooling, resistive cooling, dE/dx cooling, and radiative cooling (important for electrons and positrons in plasma type collectors). Power estimates for production are assessed. Paper 5, by Takahashi and Powell, discussed the possible enhancement of antiproton production by multiple collisions in a thick target, suggesting a factor of 3-4 increase over single collision rates, with accompanying reductions in primary beam current or the energy cost of producing antiprotons. This scheme depends, among other things, on a class of collection devices capable of accepting very large momentum spreads (also discussed in paper 7).

Paper 4, by Larson, considered the pragmatic engineering involved in large scaleup issues, and discussed use of electron cooling as a possible special way to cool ~10^{14} antiprotons per second in real time. The proposal suggests use of a very large dedicated cooling ring and very intense (~100 KA) electron cooling beams. The paper discussed the theory of electron cooling; cooling time constants; scaling issues; technological issues;

plasma cooling; and topics for additional consideration. Engineering issues for this cooling effort were reviewed.

Presentation 11, by Mills, described in substantial detail R&D topics which can be pursued to support large-scale production and collection of antiprotons. Some 18 topics were covered, constituting the basis of a comprehensive research program (some of which would be relevant to maximizing advanced hadron/kaon facilities). Table 1 below lists the 18 topics by title. Topic 17 is relevant to paper 8, by Cline: studies of large momentum acceptance storage rings, which may make possible storage of $\sim 10^{15}$ - 10^{17} antiprotons.

Observations from Group I Activities

- There are several alternative routes to a U.S. low-energy antiproton facility, at BNL or FNAL, delivering $\sim 10^{14}$ antiprotons per year at ≤ 50 KeV. Direct routes might enable such a facility in three to four years at a projected cost of 15 to 25 million dollars.
- The time is ripe to prepare a formal proposal for such a facility and to push for its speedy construction, in view of the great potential for new and unexpected physics discoveries, and insights into applications, suggested by Groups II and III.
- The notion of portable storage rings is alluring, and construction should be sought. Their uses would be manifold, and such rings would be an enabling tool to bring antiprotons for experimentation to any suitable laboratory in the United States.
- An advanced hadron/kaon facility has compelling physics motivation, and is the subject of four separate proposals worldwide. Such a facility would permit a potential factor of $\sim 10^3$ - 10^4 scaleup in antiproton delivery over the yields from a first U.S. low-energy antiproton facility, by cleverly exploiting the basic machinery such a facility would possess for its primary mission.
- Issues inherent in another factor of $\sim 10^3$ scaleup (to milligrams per year levels) were assessed; the consensus was:
    - The necessary accelerators can be built, selecting from several options.
    - Targetry can be scaled up, with appropriate R&D.
    - Cooling is the most serious problem, needing intensive study and innovation.
- One possible solution to the cooling problem at milligrams per year delivery levels lies in electron cooling, and one such specific cooling embodiment was discussed.

- A comprehensive RDT&E program treating issues to achieve milligram per year antiproton delivery levels can be formulated (see Table 1). Outputs of this program, a substantial portion of which is investigatable in the next five to seven years, could benefit improved designs of advanced hadron/kaon facilities, as one possibility.

Table 1

POTENTIAL RESEARCH AND DEVELOPMENT AREAS TO SUPPORT LARGE-SCALE PRODUCTION OF ANTIPROTONS AT RATES OF MILLIGRAMS PER YEAR

1. Antiproton production cross sections in heavy nuclei
2. Energy deposition in heavy metal targets
3. Positron production in heavy metal targets
4. Target hydrodynamics
5. Target materials studies
6. Plasma collection lenses
7. Large aperture collector rings and beam transport
8. Plasma lenses for collectors
9. Intermediate energy electron cooling
10. dE/dx cooling
11. Combined electron and stochastic cooling
12. Passive electronic cooling
13. Future electronics for stochastic cooling
14. Simulation of collider collectors
15. Intense rapid cycling synchrotrons
16. Intense high repetition rate linacs
17. Scaleup of antiproton transport storage rings
18. Future workshops

IV. SUMMARY OF GROUP II ACTIVITIES, REFERENCED TO NUMBERED PRESENTATIONS

Note: Group II presentations give as upper bounds for the numbers of antiprotons available the amounts an *initial* U.S. low-energy antiproton facility can deliver: $\sim 10^{14}$ antiprotons per year.

The basic physics case for low-energy ($\leq$ 200 MeV) antiproton research is compelling. The diversity of the physics involved is broad, and was summarized in paper 1, by Bonner and Nieto: Tests of CP, CPT, and T invariance principles; gravity and antiprotons; antiproton annihilation in nuclei; antihydrogen and basic physics tests; meson spectroscopy; antimatter storage in normal matter; and tests which invoke higher energies (up to several GeV) for the antiprotons: CP violations in $\bar{p}p \rightarrow \bar{\Lambda}\Lambda$, charmonium spectroscopy, and branching ratios in J/ψ and ψ' decays, where very large deviations from quantum chromodynamics predictions arise. The higher energy tests should be accessible in any facility whose main function is to produce low-energy antiprotons. The comprehensive overview paper 1 summarized the arguments for a low-energy antiproton facility, and in its Table 2, reproduced here, suggested the number of antiprotons needed for 12 classes of experiments.

Antiproton annihilation in nuclei (paper 2, by Smith) in a low-energy facility reveals fundamental insights into production of very high nuclear temperatures; provides information on deep annihilation, strangeness and quark-gluon matter, and production of $\bar{N}NN$ fireballs; and exploits fission as a new tool for studying strangeness of heavy nuclei. For example, antiprotons - nucleus collisions allow exploration of the high temperature region of the nuclear phase diagram. The particle emission from antiproton annihilation (paper 3, by Smith) is important in determining the fraction of the total annihilation energy release going into heavy charged particles, critical for use of annihilation energy as a propulsion or compact energy storage source. The paper suggested a greater than previously predicted value for this fraction.

Paper 4, by Sharpe, discussed the phenomenology of exotica and meson spectroscopy in the $\bar{N}N$ channel, and concluded that annihilations can help us understand strongly coupled field theory, that $\bar{p}p$ provides a good general purpose detector, and that $\bar{p}p$ annihilations will be an important tool for unravelling the exotica and provide insights into whether QCD is the correct strong interaction theory — and, if not, what might lead to a better theory. A variety of experiments has exhibited resonances which do not fit standard patterns. A high luminosity, low-energy antiproton source can play a central role in new quantitative tests.

Antiprotons are useful for testing invariance principles (CP, CPT, T) both in their role as antiparticles and as a source of other particles (paper 5, by Miller). Many types of tests are possible. Paper 5 consolidated previous test results, suggested new tests using antiprotons, and derived estimates for the number of antiprotons which might be needed to obtain precision tests with good statistics. Up to 10^{12} - $10^{14}/10^{15}$ antiprotons might be

Table 2

Summary: CHARACTERISTICS OF LOW-ENERGY ANTIPROTON EXPERIMENTS, Group II

Experiment	*Implementation Needs*[a]	*No. $\bar{p}$'s Required*	*Portable?*[b]
1. $\bar{p}p \rightarrow \bar{\Lambda}\Lambda$, CP violation	Great	$\geq 10^{14}$	No
2. K°, $\bar{K}$°, CP, & T violation	High	$\geq 10^{14}$	No
3. Inertial $\bar{M}$ = ? CPT test	Low	Few	Yes
4. $\bar{H}$° spectra, Lamb, Ry? CPT	High	10^{12}	Yes
5. Gravity: $g(\bar{p}) = g(p)$?	High	10^{10}	Yes
6. Hadron spectroscopy, exotica?	High	10^{12}	No
7. $\bar{p}$-A: quark-gluon plasma	Low	Up to 10^{14}	No
8. $\bar{p}$-A: strange fireballs, etc.	Low	Up to 10^{14}	No
9. Cold $\bar{H}$, $\bar{H}_2$, $\bar{H}^-$ . . . production & manipulation	High	Few to 10^{12}	Yes
10. Cold e^+ plasma + $\bar{p}$'s	High	Few	Yes
11. Matter/antimatter collision dynamics	Low	$>10^6$	Yes
12. Condensed matter studies:			
a. $\bar{p}$ atoms	Low	10^6	Yes
b. $\bar{p}$ channeling	Low	10^6	No?
c. $\bar{p}$'s in dynamic traps	Great	10^6	Yes

[a]Degrees of implementation needs:
Great = We have concepts, but details need intensive planning.
High = It's hard, but we know how.
Low = State of the art.

[b]Where "No" is stated, check possible use of portable ring intermediary.

desirable, thus emphasizing the high motivation for an intense source in a U.S. initial low-nergy antiproton facility.

Gravity experiments with antiprotons (paper 6, by Nieto) are of fundamental importance and are motivated in part by apparent non-Newtonian, non-Einsteinian effects suggested by recent experiments, reanalysis, and other work, and in part by quantum gravity, which suggests vector and scalar partners of the graviton and consequently additive contributions to the Newtonian potential for antimatter, whereas for matter the partners' contributions have opposing signs and hence may nearly cancel. A prediction, based on use of recent mine data that imply possible magnitudes for the scalar and vector coupling constants and for the force ranges of the additive contributions, suggests that antiprotons may fall to the earth 10% faster than normal matter. The experiment suggested uses of the hydrogen ion as a calibration, leading to precision measurements. This experiment is assuming greater and greater potential importance in view of the many other changes in thinking on fundamental aspects of gravity in recent years.

The possible storage of antiprotons in relative proximity to normal matter was discussed in paper 7, by Campbell. Whereas equilibrium storage appears impossible, a variety of schemes for steady-state non-equilibrium storage in a wide spectrum of condensed matter systems cannot now be ruled out. Known limits to stability were discussed, as are down-scaling of macroscopic traps; condensed matter traps; special effects relying on a variety of quantum mechanical mechanisms; and experiments with antiprotons in condensed matter. Muons would likely serve as useful test particles in such fields as developing very small scale traps.

Antihydrogen ($\overline{H}$) production schemes were reviewed in paper 8, by Mitchell. Schemes include stimulated radiative recombination, positronium charge exchange, and high-density three-body recombination in a trap; with modest technology advances, production rates of $\geq 10^8$ antihydrogen atoms/sec seem attainable. $\overline{H}$ production is necessary to provide a possible basis for very high density storage of antimatter, vital for many proposed macroapplications of antimatter (e.g., propulsion). Basic physics uses of $\overline{H}$ are also exceedingly numerous, e.g., every measurement made with hydrogen would have repetitions with antihydrogen vital to CPT predictions. Normal matter simulations of $\overline{H}$ production can be exploited.

The cluster ion production technique of macroscopic amounts of antimatter was described in paper 9, by Stwalley. This technique can have significant implications for storage of bulk amounts of antimatter. The paper first discussed the formation processes, efficiency, etc. for normal matter and then considered complications when antimatter is used. The scheme considers producing $\overline{H}$ and a catalyst $\overline{H}_N^+$; the individual reaction steps potentially leading to the $\overline{H}_N$ "seed crystal" were reviewed in some detail. Processes leading to bulk amounts of antimatter were then described. Normal matter simulations can be envisaged; normal matter cluster ions are themselves of substantial scientific interest, and of potential importance in producing particle beams for directed energy, fusion, solid state, and other applications.

An extensive bibliography of hydrogen cluster ions was given in paper 10, by Stwalley. Over 400 listings discuss formation issues for H_2^+, H_3^+, and H_N^+ ($N \geq 4$) in turn; in addition, the H_2^-, H_3^-, and H_N^- species are reviewed (H_2^- is unstable, and probably so is H_3^-). The richness of the experimental and analytical work suggested by this bibliography will give us a running start on antimatter cluster ion research.

Paper 11, by Forward, discussed experimental work resulting in production of antideuterium, antitritium, antihelium, and prospects for even heavier antinuclei such as antilithium. Results give production rates of heavy antinuclei, normalized to production rates for antiprotons, as a function of the mass of the antinuclei and as a function of particle energy. Each added baryon, for example, appears to lower the production rate by a factor $\sim 10^4$. Production of heavy antinuclei is of very considerable scientific interest and usefulness in itself; in addition; heavy antinuclei might play a role in antimatter cluster ion research.

Paper 12, by Goldman, discussed the physics issues which can be investigated via an Advanced Hadron Facility, and thus comprehensively reviews the primary physics justification for the facilities described in papers I9 and I10 under Group I activities. Paper 12 considered the fundamental particles and gauge bosons; strong interaction theory; the standard electroweak model; and problems of the standard model and consequent experimental tests. Precision experimental tests require high intensity, medium-energy (~30-75 GeV) accelerator complexes to meet the experimental needs. Such an accelerator complex would be a means for a substantial scaleup (by a factor of $\sim 10^3/10^4$, say) of antiproton production and collection, compared with current and near-term antiproton sources. This feature is the motivation of the discussions in papers I9 and I10.

Paper 13, by Nieto and Hughes, summarized the evolution of thought on antimatter and its roles in modern science.

Observations from Group II Activities

- Opportunities are abundant for explorations with low-energy antiprotons.
- New discoveries and exciting results await in tests of invariance principles, antiprotons and gravity, annihilation phenomenology, meson spectroscopy, antihydrogen and basic physics tests, antimatter cluster ions, antimatter storage in normal matter, and production and use of heavy antinuclei.
- We need intense sources of low-energy antiprotons to achieve such discovery goals.
- Even for basic science, there are classes of experiments which would exploit the upper portion of the near-term capacities of prospective low-energy antiproton sources in the United States ($\sim 10^{13}$ to 10^{15} antiprotons/year).
- LEAR has only scratched the surface of compelling, attractive, low-energy antiproton experiments. There is plenty of work for another low-energy machine in North America (also available for international collaborations).
- A low-energy antiproton facility, such as the one under consideration in this Workshop, can address major areas of concern in particle physics today, as emphasized both here and in the Fermilab Proceedings (April 1986), in a vital and straightforward way. The diversity of the physics discussed by Group II is broader than that of the Fermilab 1986 Proceedings.
- Many of the aims of the program of basic science experimentation discussed by Group II appear generally compatible with, and often expeditable by, use of transportable antiproton storage devices — ion traps (see paper III1) and small rings (see paper I8). The basic antiproton source (see papers I1, I2, I3, I6) would be a filling means to load the transportable storage devices; the actual experiments would be performed in any competent laboratory.

V. SUMMARY OF GROUP III ACTIVITIES, REFERENCED TO NUMBERED PRESENTATIONS

Note: Group III presentations use as upper bounds for the numbers of antiprotons available the amounts an *initial* U.S. low-energy antiproton source can deliver: $\sim 10^{14}$ antiprotons per year.

Paper 1, by Howe et al., discussed the principles of and a point design for a large portable ion trap storing $\sim 10^{13}$ antiprotons at 25-50 KeV. The design is conservative (e.g., a factor of 100 down from the Brillouin limit, compared with the National Bureau of Standards experiments, which are a factor of 30 down). The particles are confined in a cylindrical plasma volume 200 cm long and 5 cm in diameter; the vacuum is $< 10^{-12}$ Torr, giving a storage time of ~30 to 100 days or better; the magnetic field is 10 T. A complete installation, including all support equipment, can easily fit into a large truck. Replicating the trap design might cost 200,000-500,000 dollars, once the design has been validated. Shielding requirements were assessed as was whether an emergency plasma dump into an absorbing target was feasible, alleviating needs for 4π radiation shielding around the entire trap. R&D topics identified include vacuum requirements, need for confinement data, whether feedback can nullify slow radial losses and so forth. The point design can be scaled to smaller storage levels and very compact storage assemblies.

Paper 2, by Solem, discussed the general theoretical basis for opacity and equation-of-state measurements. The basic question here is whether antiprotons can be used for experiments in extreme states of matter without the need for large and expensive centralized facilities available to relatively few researchers. A "table-top" tool using antiprotons from a portable storage device would open up the research area to a much wider audience. The main areas of interest include high temperature, high pressure, high secondary particle (pions, γs, etc.) flux research, and work such as that described in papers II2 and II3.

In the high temperature area, interest centers around opacity or radiation transport measurements. Classical opacity measurements involving destroying a target in a spherical cavity and observing the emergent black body radiation front can be adapted to an antiproton driver, but the energy requirements for a table-top device are high. On the other hand, non-classical experiments using the heat capacity of the target for energy storage appear more feasible, with foreseeable near-term antiproton technology; one such was described in some detail.

The case for equation-of-state experiments (looking at the interdependence of thermodynamic variables at high pressure) was reviewed. Using 10^{13} to 10^{14} antiprotons and challenging pulse characteristics, a shock pressure of 55 mbar could be obtained. This is quite competitive with the best nuclear-explosive-driven and laser-driven experiments. However, it too is stressful on foreseeable antiproton technology, and on pulse characteristics attainable (e.g., 1-10 nanoseconds).

Assuming a small storage ring with 10^{10} 100 MeV antiproton capacity, a table-top driver with 10^{15} pions per cm^2-sec was described in paper 3, by Mayer. Although no specific experiments were discussed, there was general agreement that this could be a useful capability. Such a driver is scalable, and can serve as an interesting source for a number of external particle flux experiments.

If these challenging technology characteristics can be met, a table-top antiproton tool would open areas of fascinating scientific and applications research. Paper I11 discussed one such research topic.

Paper 4, by Morgan, described some of the information base necessary to critically evaluate, and perform realistic conceptual and implementation designs for, antimatter propulsion engines (rocket or air-breathing).

The promise of using antiprotons in propulsion awaits not only order-of-magnitude increase in antiproton production, but also a better understanding of how antiprotons and their annihilation products interact with matter. The propulsion talks dealt primarily with the latter, and revealed a wide variety of experiments that would provide data for that understanding. Experiments described in paper I13, for example, are clearly relevant.

Proposed experiments with antiprotons available from a low-energy antiproton source that were directly relevant to rocket engines concentrated on potential problems and limited areas of understanding for the four basic engine types: solid core, gas core, plasma core, and beam core. Adapted forms of such engines are also relevant to air-breathing engines. The two main issues for these engines are: (1) getting the antiprotons to annihilate where you want them to, and (2) getting the annihilation energy deposited where you want it. A variety of experiments were described, involving stopping distances and annihilation cross-sections of low-energy antiprotons in unionized matter; annihilation energy deposition; and a number of engine models. How such experiments contribute to full-scale engine model design is critical, involving assessment of basic feasibility, code verification and calibration, design optimization, evaluation of radiation phenomenology and shielding, and the like. Such information is necessary if we are to assess engines realistically.

Paper 5, by Callas, described a generic experimental apparatus with which many antiproton engine-related processes could be investigated, using modifications of current high-energy particle detector technology. The paper identifies key research issues, and uses the proposed experimental apparatus with quantities of antiprotons consistent with quantities deliverable from assumed low-energy antiproton facilities.

Paper 6, by Cassenti, discussed the systematic attributes of a specified class of engines, and establishes the efficiencies attainable with magnetic deflection in a vacuum, effects of propellant density, and so forth. A parametric study shows effects of propellant choice, mass ratios, and magnetic fields over wide ranges.

Papers 7, by Rafelski, and 8, by Takahashi, described fundamental aspects of antiproton annihilations interacting in a DT mixture, in a given conceptual engine embodiment which exploits muon catalyzed fusion. The chain of reactions possible here may amplify the basic annihilation energy release by a factor of ~5. The conceptual engine supposes a lithium mantle for tritium production, and exploits previous Monte Carlo simulations to prescribe some suggested target/fusion vessel parameters. Associated with presentations 7 and 8 was a presentation by Maglich on a self-collider proposal. The group of presentations also discussed possibilities for scaling up antiproton production (to about a gram/year level) and ideas for production of heavier antielements, such as antilithium.

Paper 9, by Haloulakos, noted that national programs are making available laboratory facilities to routinely generate H^-, H^+, etc. These particles can be used in a number of ways relevant to propulsion experiments, e.g., working with slush hydrogen, using particle beams as heaters, and the like.

Paper 10, by Kalogeropoulos et al., introduced what may be one of the most compelling near-term and high-payoff applications for low-energy antiprotons — medical uses.

Experiments with low-energy antiprotons were discusssed in three general areas of medicine: dE/dx imaging, therapy, and antiproton mesic chemistry. Portable storage devices can be exploited.

Imaging appears to be perhaps the most promising single near-term application for antiprotons. As an example of the potential of antiprotons, 10^7 antiprotons could give the same quality image as a computer tomography scan, with 1/15 the dose and none of the artifacts that can cluster in a CT image. An entire image requires only 10^9 antiprotons, which is also well within the portable storage capacities envisioned.

For therapy the doses must be increased one or two orders of magnitude, and at those levels more information is needed about the local energy deposition in biological targets. One potential application for antiprotons in therapy is as a tool for testing, monitoring, simulating, and improving proton and heavy ion therapies. Because antiprotons annihilate at the end of their range and send out products that can be traced back to the annihilation point, they are unique among portable particle beams in their ability to determine accurately where the therapeutic effects are taking place.

The third interesting area for medical experimentation with antiprotons, using x-ray emissions or nuclear gammas, is in the general area of "mesic chemistry" or imaging elemental atoms in-vivo or in-vitro. Antiprotons have several advantages over muons used for the same purpose and, with portable storage devices, promise the ability to monitor all elements in the living body. Oxygen, carbon, hydrogen, nitrogen, calcium, and phosphorus — in fact, all elements at once — can be imaged by events with 10^9 antiprotons (i.e., ~1 rad), with images of constituents up to phosphorus made with millions of events.

Preliminary experimental trials of these biomedical applications can be undertaken at BNL (or LEAR); the requisite detector cost is estimated at ~600,000 dollars, and an operational direct budget cost is estimated at ~300,000 dollars per year. Note that for these preliminary trials the current alternating gradient synchrotron low-energy separated antiproton beam can be used (~10^7 stopping antiprotons per hour).

Presentation 11, by Koehler, presented data on the relative stopping power of organic compounds; various tissue mass stopping powers relative to water; typical correlations, for various tissues and blood constituents, of measured stopping power and density; and typical calibration means. Such measurements are relevant to issues discussed in paper 10.

Presentation 12, by Archambeau, described, from a practicing clinical perspective, the varieties of clinical applications considered in an upcoming proton therapy facility at Loma Linda University Medical Center, and briefly described of some of the small proton machine characteristics (e.g., 20 ft diameter, 70-250 MeV energy range, ~20 nA beam current). Such a facility could be a model for a corresponding antiproton facility in an operational mode.

Presentation 13, by Ottewitte, summarized much pertinent physics data, compared antiprotons to other types of probes, and raised a number of application possibilities.

The paper dealt with using antiprotons in scientific and commercial diagnostic probes, tools, and special techniques. Such instruments may have applications in many fields of normal matter research, and could span such uses as vacuum measurements, plasma diagnostics, material analysis and treatment, and special radiation characteristics. The paper gave illustrative calculations, and noted additional areas and topics for further study. This area appears to have been underinvestigated.

Paper 14, by Greszczuk, reviewed suggested uses of antiprotons for quantitative non-destructive evaluation (NDE) of materials, measuring local densities and density gradients; new material processing techniques; defect healing in materials; and identification of material compositions. These uses have analogues in biomedical applications (papers 10, 11, and 12). One potentially important industrial use employing amounts of antiprotons available in the near term is illustrated by an example comparing use of computer tomography (CT) and antiprotons, in terms of inspection speed, for inspecting a critical component (a carbon-carbon exit cone). The comparisons suggest that use of antiprotons might speed up this process by a factor of ~1000 (i.e., CT time ~12 hours, antiproton time ~11 seconds). There is thus substantial motivation for fuller assessment of such uses of antiprotons as soon as a low-energy antiproton facility becomes available, for potential industrial/military benefits.

Paper 15, by Forward, reflected an intensive bibliographic search on the 10 major topics identified, brought up to a date of August 1987. Interested readers and researchers can thus access information of direct interest.

Observations from Group III Activities

- The technology seems ripe for developing a family of portable ion traps, complementary to use of portable storage rings, for storing antiprotons in amounts up to $\sim 10^{13}$ particles, thus allowing transport to and use at laboratories removed from FNAL or BNL.
- A number of potential applications-oriented uses for antiprotons, employing antiproton amounts deliverable by a first U.S. low-energy antiproton source ($\sim 10^{14}$ antiprotons per year), appear attractive and worthy of further study.
- Basic tools, experimental procedures, instrumentation, and the like for applications-oriented research are comparable to those needed for basic science work. We expect these two streams of effort to reinforce each other.
- As with the basic science case, the possibility of pursuing near-term, useful micro-applications research emphasizes the vital needs for a U.S. low-energy antiproton source and development of associated enabling tools, such as portable storage devices.

- By joint pursuit of both the basic science and microapplications research, one can envision fast progress in assessing the feasibility and utility of many proposed large-scale uses of antimatter considered to date.